

**From:** [PETERSON Jenn L](mailto:PETERSON_Jenn_L)  
**To:** [Eric Blischke/R10/USEPA/US@EPA](mailto:Eric_Blischke/R10/USEPA/US@EPA)  
**Subject:** RE: Food Web Model  
**Date:** 08/04/2008 10:57 AM

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Hi Eric,

I have been meaning to call you, I just have to finish up a few other things. I will give you a call early afternoon with these issues in mind.

-Jennifer

-----Original Message-----

From: Blischke.Eric@epamail.epa.gov  
[mailto:Blischke.Eric@epamail.epa.gov]  
Sent: Monday, August 04, 2008 10:54 AM  
To: PETERSON Jenn L  
Subject: Food Web Model

Jennifer, I would like to discuss the following elements of the food web model comments with you:

1) How to handle temperature. I understand what the LWG did and your comment. I also understand that this is a sensitive parameter that varies throughout the year and has an impact on metabolic rate, feeding rate and chemical specific parameters. However, I am not sure what we should tell the LWG to do. It seems that we should attempt to match the seasonal distribution of the temperature data in the Willamette River somehow.

2) Water data: My read of the model (and previous agency comments) is that we told the LWG to use the empirical data dissolved water data rather than calculating the bioavailable fraction from the total data. The LWG applied the 1997 Morrison equation to go from dissolved to bioavailable with a correction based on the difference between 0.2 and 0.5 micron filter sizes. At this point, I am really not sure which approach is better. The question really is which method - Arnot and Gobas; total to bioavailable or Morrison; dissolved to bioavailable - imparts more uncertainty. I really do not care about one value being 1/3 of another. I want the best estimate.

3) Pore water ventilation rate. My main question is what do Arnot and Gobas recommend. I was on the internet looking for information and could only find the San Francisco Bay FWM which used a benthic pore water ventilation rate of 5%. The LWG used 5% and varied it from 1 - 10% and assumed a porewater ventilation rate of zero for the species that we told them to assume zero for. I am not sure I understand, or that this point, agree, with your comment. If Arnot and Gobas recommend 5%, I think the LWG approach is appropriate.

4) Uncertainty and Parameterization: When I read your comment regarding uncertainty and Table 3-7, the issue seems to more an issue with parameterization than uncertainty. I need to understand better the difference between the MB and SPAF approaches to settle the uncertainty issue. Regarding parameterization, I have outlined an approach where we take the model as the parameterized LWG model and see how it works with the Round 3B data and then decide how to proceed. My perspective may be a little different from the reviewers. I do not care so much about the parameterization if it can get us a more reliable tool for understanding the relationship between tissue concentration and sediment levels recognizing that any relationship developed for a large site with numerous sources and a dynamic river environment is fraught with uncertainty.

I would be good if we could talk later today about these four or five topics. I am available pretty much all day.

Thanks, Eric